PROCEEDINGS

OF

THE ROYAL SOCIETY.

1833–1834. No. 15.

December 5, 1833.

JOHN WILLIAM LUBBOCK, Esq., M.A., V.P. and Treasurer, in the Chair.

James Copland, M.D.; Edwin Pearson, Esq., M.A.; and Charles Terry, Esq., were elected Fellows of the Society.

December 12, 1833.

FRANCIS BAILY, Esq., Vice-President, in the Chair.

The Reports received by the Secretaries, from Sir John Herschel, Professor Airy, and Captain Smyth, on the Fluid-lens Telescope constructed for the Royal Society on Mr. Barlow's principles, were, by direction of His Royal Highness the President and Council, read to the Society at this meeting.

Sir John Herschel's Report.

I have seen Mr. Barlow's telescope at Cambridge, and examined it on several objects, in a very fine night, the 25th (if I remember) of June. As I have now no time to give it any further trial at Slough, (where I have no longer, either, any achromatic telescope of sufficient power to compare it with, all my apparatus being dismounted and in course of packing,) I will here state in few words, as *my* report on it, all I could then collect relative to its action.

1. *Achromaticity.*—Mr. Barlow's telescope is remarkably free from the dispersion of colour, very much more so than I could have expected from the nature of the correcting medium, and nearly or quite as much as could be desired.

2. *Light.*—The great aperture is very efficient under moderate powers on faint objects; and it concentrates the smaller stars well, and would, I have no doubt, show the larger nebulæ, &c., and be well available as a *sweeping telescope.*

3. *Distinctness.*—Very good with powers under 100 or 150; but on the occasion on which I tried it, it seemed to break down under high powers, and there was evidently a considerable want of correction of spherical aberration. As I had no opportunity of trying it in different temperatures, I cannot say whether this want of correction might not disappear in a different temperature,—it was about 65° when I looked through it,—neither could I ascertain whether this

arose from the glasses not being at the right distances, there being no means, or the means not having been explained to me, by which the correcting lens could be got at, to shift it.

4. A very troublesome degree of colour out of the centre of the field.

This report is of course too meagre and imperfect to conclude much from, but as both Capt. Smyth and Prof. Airy have examined it in much detail, I the less regret that my present circumstances will not allow of my going further into the subject.

J. F. W. Herschel.

July 23, 1833.

Professor Airy's Report.

From the pressure of business I have had fewer opportunities of trying the telescope than I could have desired. The absence of bright planets also has prevented me from attending so much as I wished to what I regard as the most important point in this construction, namely, the correction of colour. I have, however, had one excellent opportunity of observing the moon, and have observed several stars, single and double, and do not think that my opinion could have been altered by a greater number of observations. The correction of colour is not complete, but it is much more nearly complete than I expected, and very much more so than in a smaller telescope of Mr. Barlow's construction which I tried several years since. The colour is so far removed that it is not offensive till a power of 300 is used. But with regard to this colour, there is one point of great importance to be noticed by any person who shall try the telescope in future. It is that, in consequence of the separation of the object lenses, the only part of the field which can possibly be free from colour with a common eyepiece is in the line passing through the centres of the two object lenses; and that from the present imperfect centering, this line falls actually out of the field of the highest power (or quite on the edge). An eyepiece of a different construction and adjusted with greater care is necessary before any positive decision can be given. With regard to the definition of a star, it is not at present good, and the telescope is decidedly incompetent to separate any close star; but I regard this as a fault in the making of the surfaces, to which any telescope is liable, and which does not interfere at all in my estimation of the value of the new principle of construction. I know not how far a circumstance mentioned by Mr. Dollond (the alteration of spherical aberration with an alteration of temperature) may account for this; but so much of the irregularities are cut off by cutting off the external ring of the object glass, that I have no doubt of its being due principally to the figure.

My opinion is, therefore, that a larger telescope, as good of its kind as the present, would be very useful for nebulæ, &c.; and that if freed from defects, which do not appear to belong to the construction, it might be equal to any astronomical work except the examination of bright planets.

I have had the advantage of trying the telescope once in company with Sir John Herschel, Sir David Brewster, Mr. Cooper, Dr. Ro-

binson, and Professor Hamilton, and their opinion upon the whole coincided nearly with mine.

I beg to suggest the propriety of attaching a finder to the telescope, as much time is lost in seeking for any object.

G. B. Airy.

June 20, 1833.

Captain Smyth's Report.

I beg you will inform His Royal Highness the President, and the Council of the Royal Society, that I have this day packed up the fluid refracting telescope of which they have done me the honour of asking my opinion, and that it will be forwarded to Professor Airy without delay. It might indeed have been sent to Cambridge sooner, but that I waited for the first quarter of the present moon, to test the light and the performance of the instrument; but I regret that though I was constantly upon the spot, the weather has prevented my having an opportunity of catching her, till she was past her dichotomy, and consequently too glaring for the purpose.

On the arrival of the telescope, it was carefully unpacked, and immediately mounted, for the moment, on the lower slab of the revolving roof of my polar-axis room. It was fitted by its two pivots to the iron crutch which was sent with it, the upper parts of which were cut into Y's: the inner end was supported by Mr. Dollond's ingenious "eye-end stand." The instrument, however, was liable to tremor, both from the motion of the roof and the floor; but it enabled me to examine a few objects while poles were being prepared to form a better stage outside the observatory. And I should remark, that it was arranged with Professor Airy, who favoured me with a visit on the occasion, that my experiments were to be entirely confined to the performance of the telescope, while he would investigate its principle. My portion was to be governed by direct comparisons with my refractor, as a standard from which to assume the relative merits of the two. That instrument has a double object-glass of $5\frac{9}{10}$ths inches clear aperture, and $8\frac{1}{2}$ feet focal length; a space which I have good reason to think is accurately proportioned to the densities of the crown and flint glasses: and notwithstanding the magnitude of the diameters, the curves of the lenses seem in tolerably exact chromatic and spherical aberration throughout. It may therefore be presumed to be a more severe reference than the dimensions alone would suggest.

The temporary stage alluded to, outside the observatory, consists of two upright beams of fir, firmly driven into the bed of gravel which forms the substratum of the garden, and a cross-bar, strongly screwed, supports the iron crutch with its Y's. This is erected close to a platform and pier, which were built for some magnetic experiments, and afforded great facility in attending to the outer lens, and augmenting or diminishing its aperture. While looking towards the south, it commanded from nearly a horizontal view to above 60° of elevation; and by unshipping it, and turning it northwards, it swept the polar region. Such being the means, it remains faithfully to report what I observed, regretting, at the same time, that the weather has continued mostly unfavourable.

Monday, Feb. 25, 1833.—The evening cleared off, and was very fine from 8 till nearly 11 o'clock P.M. At 7, I placed the instrument on its stand; at 9, the thermometer was 37°·6, the barometer 29·32, and the hygrometer ·771; and the wind was at S.E.

1. *The Moon.*—The examination of the lunar cavities and shadows was rather unsatisfactory. Under the powers 250 and 400, it bore the whole aperture; but with 90 and 150, there were two spectra, one of which haunted the centre. In definition, the fluid was excelled by the flint-glass, both instruments being very steady.

2. *The great Nebula in Orion.*—This mass was seen very fairly with the whole aperture; and the trapezium was beautifully distinct under all the powers except that of 400. From the examination of this object, the best performance seems to be with the eyepieces 150 and 250. The relative light of the flint-glass and the fluid-refractors, when the latter was reduced to six inches of aperture, appeared very nearly equalized.

3. *Venus.*—This trial was altogether unsatisfactory, from the strong irradiation and the quantity of loose light. The planet was, however, low down in the west, in a stratum of mist. The only power used was the one of 90 times; but there was a great defect in distinctness.

4. *Rigel.*—This star was in the S.W., and rather low; it was therefore, as might have been expected, surrounded with teasing rays, through which I had some difficulty in detecting the small companion. The star had a spurious but broken disc, and was full of colours in every part of the field except the centre, where they were partially destroyed. Powers 150 and 250.

Tuesday, Feb. 26, 1833.—At 9 in the morning, with the thermometer at 38°·8, and the hygrometer ·798, I examined an enamelled watch-face, which is firmly fixed upon a distant chimney of solid construction; and though the solar focus could not be used, I considered sufficiently distinct vision would be obtained to test the achromaticity of the telescope. The plate itself bore the trial better than did the edges of the chimney-sides, where the focus could not be adjusted so as to prevent the alternate production of light green and purple mist, as the eye-tube was pushed in or out: and these colours scarcely disappeared, even when brought into the centre of the field of view. Some of this might probably be corrected by adjusting the fluid-lens for near objects: and Professor Barlow writes to me, that this can readily be done; but that he took off the screw-head, by which it is effected, to prevent mere lookers-on from deranging the instrument. The watch-face being upon a dark ground, I played the eye-tube till I procured a spurious disc over it, by which I was satisfied that the centering was very nearly accurate.

I then left the telescope *in statu quo*, and at half-past 12 again inspected it, the thermometer being 47°·6, the barometer 29·23, the hygrometer ·789, and the wind S.S.W.; particulars which I carefully noted, for a reason which will presently appear. The watch-plate was now considerably plainer, and its figures more sharp and distinct; but the focus required shortening in; and though there was less colour than before, I was surprised to find it verging to the prismatic extreme, and tinged with red; a circumstance which ocular foci, or the distance,

would hardly account for. I repeated the examination in the evening, when the thermometer was 45°·4, and the hygrometer ·790. I now found that the focus required lengthening; but the vision was at its best, and the colours had almost vanished, though a foggy spectrum was perceptible at times. High powers, of course, did not agree with so near an object; but they were used without greatly distorting the image.

Thursday, Feb. 28, 1833.—The weather was very fine from 10 to 12 P.M., though the wind blew occasionally in hard squalls from the S.W. The instrument appeared but little affected, and yet the observations were rendered unsatisfactory by the frequency of these gusts. At 11 the thermometer was 38°·4, the barometer 29·45, and the hygrometer ·723.

1. *σ Orionis.*—Saw 8 of the 10 stars which compose this cluster, but not sharp. The situation was unfavourable, it being two hours and a half off the meridian, and the S.W. quarter of the heavens was hazy. The power used was 250.

2. *ζ Orionis.*—This, of course, was very plainly seen; but I fished it up for its definition. The large star had a formidable nimbus, yet it did not prevent the increase of dark vacancy on raising the magnifying powers. There was much less loose light than I expected, and the small star was palpably of a pale-blue tint.

3. *Rigel.*—This star was now too far in the S.W. to be made much of: it was tremulous, and greatly irradiated under power 250. The companion was not visible, and there were two troublesome spectra.

4. *Saturn.*—The body of the planet bore magnifying powers, and showed the thin silver line of ring which now appears, without distortion, but certainly without sharp definition. I could only perceive two of the satellites, while with the flint-glass refractor I saw three. The whole aperture was too much for the instrument, and it was therefore cut off to six inches.

5. As the north was now the clearest part of the heavens, at about 11 o'clock the telescope was turned to that direction. The pole-star and its companion were seen very distinctly, even under the lowest power. This, of course, I expected; but I found that it was also viewed on both sides of the object-glass, with much less colour than the other tests I had been looking at.

Monday, March 4, 1833.—This was the best night I had yet had, and it continued very fair till nearly midnight. I was somewhat troubled with dew, but the instrument was free from tremors, and worked as well as its temporary mounting could admit of. The temperature stood thus:

	8 *o'clock.*	10 *o'clock.*	*Midnight.*
Thermometer	45°·5	43°·6	40°·5
Barometer	29·85	29·86	29·89
Hygrometer	·740	·737	·728

1. *Rigel.*—This star was greatly discoloured at the edges of the field, and was accompanied by a singular spectrum, which was not destroyed by being brought into the centre. I caught the companion

by glimpses, but it was immersed among strong rays. The powers used were 90 and 250.

2. *Sirius.*—This brilliant star was still more discoloured than Rigel on either edge of the field of view, and had a continuous production of rays, which in the centre surrounded the star, but at the sides preceded and followed it, like the wings seen where a flint-glass is not homogeneous, but fainter. These irradiations, as well as the dispersed light, were considerably cut off by diminishing the aperture of the outer lens. The powers used were 90 and 150; and I tried with 400 to raise a disc, but it was altogether too much for the object.

3. *σ Orionis.*—Saw the whole of the 10 stars of this group, but with great difficulty, and, if the term may be used, under a dim definition. Indeed, had I not practically known the object, I am doubtful whether I could have made out the middle stars. It should, however, be also stated, that it was nearly three hours to the west of the meridian.

4. *The great Nebula in Orion.*—I placed the whole aperture upon this object; and though the moon was nearly at full, I easily made out its outline, as well as that of its companion. But the trapezium of stars, under high powers, was more distinct with an aperture of 6 inches than when the whole was applied. I could make out only four stars in this spot; it will be recollected, however, it was now three hours past the meridian, for the time of its transit will not allow of earlier experiment. This I regret, because so fine a constellation, from its composition and place, offers in itself a *thesaurus* of astronomical tests.

5. *γ Leonis.*—This beautiful double star was remarkably well seen, being nearly on the meridian. There was, however, much false light, but it did not hinder the colours being seen: the large star was slightly red, and the small one a Saxon green. The powers used were 90, 150, and 250.

6. *ω² Leonis.*—This was a test which, in the deficient arrangement of the apparatus, I could not manage; but notwithstanding there was much dispersed light, I should pronounce that with power 400 I saw the star elongated, and different from the other two *omegas*.

7. *The Præsepe.*—An examination of this cluster was very favourable to the defining power of the telescope, and its general distribution of light. I tried it under the eyepieces 90, 150, and 250.

8. *ζ Cancri.*—With some difficulty I made out this object to be triple, under a power of 250: that of 400 broke the rings of the spurious discs with disagreeable rays, so as to confuse the whole vision.

9. *Saturn.*—The planet was about two hours and a half to the east of the meridian when I placed the telescope upon it. It was tolerably defined, but with *muddy* edges, though it bore magnifying pretty fairly. I saw two satellites steadily, and a third by glimpses; and this was all I could do with my own telescope at the time of transit. The ring resembled a thin silvery bar lying equatorially across the planetary disc, and was sharper than the body of Saturn.

Wednesday, March 20, 1833.—I had now intended to wait for the first quarter of the new moon; but the night proved so fine and dark, that I re examined some of the former tests, and observed some new

ones. There was a light N.W. wind, and the temperature was thus:

	9 *o'clock.*	*Midnight.*
Thermometer	37°·7	34°·4
Barometer	30·01	30·00
Hygrometer	·680	·670

1. *The great Nebula in Orion.*—This was now three hours and a quarter over the meridian, and yet it was seen in great beauty and distinctness under the whole aperture, with eyepieces 90 and 150. The trapezium was examined very closely with 250 and 400, which last it bore better than it had yet done; but only four stars were visible.

2. *σ Orionis.*—All the stars of this group were perceptible under the power 250, but they had the appearance of being seen in a second-rate reflector; so that I know not how a micrometer would work upon this instrument.

3. *Venus.*—The crescent which this planet now forms was better seen than heretofore, but an unseemly quantity of light still attended it; and under the higher powers the colours were intolerable. When, however, the focus of power 90 was nicely adjusted, and the planet brought exactly into the centre of the field, it was a beautiful object, despite of a secondary spectrum. The aperture was reduced, and I did not find, either now or on other occasions, that this sensibly affected the ocular focus.

4. *γ Leonis.*—This brilliant object was distinctly seen, and the dark vacancy between the stars increased more than did the spurious discs, while the magnifying powers were being raised, though much loose light and irradiations were thereby produced. And it is singular that the separation was improved by my placing a central disc of card-paper, two inches in diameter, on the outer lens.

5. *Messier's 46th Nebula.*—This was very fairly resolved into stars, and better with the whole than the reduced aperture. Eyepiece 90 showed it easily, but the higher powers gave it a very *turbid* appearance. The preceding cluster was brilliant.

6. *α Leonis.*—This star had a bunch of disagreeable rays shooting from it; and the light, when under the best adjustment I could give the focus, was curiously thrown to the northward. I was able, however, to raise a tolerable disc, and the small star at a little distance from Regulus was unusually distinct.

7. *24 Comæ Beren.*—I pointed to this remarkably pretty object to test the colours, and very readily perceived the large star to be of a bright orange colour, and the small a sea-green. This was one of the best sights I had yet had, and on the whole was satisfactory.

8. *ι Leonis.*—This, though a very close and unequal double star, was well shown, yet at times the stray light would obscure the companion. The large star was fairly figured, and the small seemed about the 10th magnitude, and of a greenish hue. It formed a fine test.

9. *Saturn.*—I had a good trial of this planet; and though the powers 90, 150 and 250 were borne, the disc was certainly not well

defined. The ring is still a mere *bar* lying across the equator: it was very well shown, as were also three satellites. When I applied the power 400, the whole field was strewed with harsh light.

10. γ *Virginis.*—This interesting star, though now so exceedingly close, was made double with 250, and very well shown; but with 400 there was great tremor and irradiation, so that the discs were often confused into one.

Saturday, March 30, 1833.—After a continuance of bad weather for several days, it cleared off a little; but in the mean time I had missed the favourable phase of the moon, for which I had been waiting. I therefore closed my examinations with the following one:

The Sun.—From the extreme volatility of the sulphuret of carbon, I was fearful of its expansion, and therefore had not yet turned the telescope upon the sun, lest the condensation of the solar rays, at the place where they traverse the fluid, should prove too much for the lenses. But on mentioning this apprehension to Professor Barlow, that gentleman assured me that an exposure of from five to ten minutes could do no mischief. I therefore this day reduced the aperture to three inches, and directed the instrument to the solar disc, when, sweeping over the luminary for about three minutes, I found the surface was quite clear of spots. On turning from it, I drew out the eye-tube, and looking at the fluid, perceived that the bubble was considerably diminished, but not so much as I had expected. This was the only time that I exposed the telescope to great heat.

These are the only experiments I have been able to make; and the season of the year, together with the inefficiency of the apparatus, have certainly prevented me from assigning exact limits to the performance of this telescope. Still, as I had immediate reference to one of the best refractors extant, I may add the following conclusions, premising, that I have not constantly noted down the performance of the latter upon each test, because my end was to pronounce upon the fluid object-glass. I should also observe, that the magnifying powers of both the instruments were equally matched, and their apertures were generally proportioned to nearly six inches: the eyepieces were thus:

Fluid refractor	90	150	250	400
Flint-glass ditto	93	157	240	416

From the result of my observations, it has struck me that this ingenious principle has strong claims to consideration for its valuable optical powers, but that, in the present stage, it is more adapted for stars than for planets; and should the application of it be tried on a larger scale, it might be made with sufficient illumination to examine the high-class nebulæ; a branch of practical astronomy which is now nearly shut against refractors. The defining power does not strike me as being so good as the light, nor does the achromatism seem to be perfect. Yet I should mention the want of focal and mechanical arrangement; that the only adjustment I had for distinct vision was by the hand, with the sliding eyepiece tube; and that slight derangements might be occasioned by the mounting and dismounting of the great tube, however carefully it was attended to.

I cannot but suspect that the performance of this telescope is affected by temperature, and that severe tests in the summer months might afford different conclusions to those which I have arrived at; but as I considered my opinion was desired on the instrument in its present state, I took no means for applying artificial heat. And, perhaps, the secondary spectrum which haunts the field might be mitigated, and the prismatic colours destroyed, by an alteration of the distance between the fluid and outer lenses; but the same consideration prevented my applying for a screw, by which it might have been effected.

But there is one condition of the instrument which, if correct, would be of greater importance than the rest, as connected with this Report. It strikes me forcibly, from the several effects I observed, that the focus has been cut too short; a defect which would seriously affect the spherical aberration of the outer or object lens and its dispersion: and this would account for the fluid refractor not performing better than the flint-glass one, without impugning the corrective powers of the sulphuret of carbon, or its skilful application by the scientific Professor.

April 4, 1833. W. H. Smyth.

A paper was then read, entitled, "An Account of some Experiments made in the West Indies and North America, to determine the relative Magnetic Forces, in the years 1831, 32, and 33." By the Rev. George Fisher, M.A., F.R.S.

The experiments of which the results are given in this paper were made by Mr. James Napier, late Master of H. M. S. Winchester. The needles were precisely similar to those used in the experiments described by the author in a former paper; and the observations were made with great care, and repeated several times at the same places; by which it appeared that the intensities of the needles continued unchanged during the whole period of the experiments; and the mean of all those made at one place was taken as the result. From these the relative forces at different places were computed, and stated in the form of a table.

A paper was also read, entitled, "On the Theory of the Moon." By John William Lubbock, Esq., V.P. and Treas. R.S.

M. Poisson, in a memoir which he has lately published on the Theory of the Moon, expresses the three coordinates of her path, namely, her true longitude, her distances, and her true latitude, in terms of the time. The author observes that the reasons for so doing adduced by M. Poisson, are the same as those which led Mr. Lubbock also to deviate from the course which had previously been always pursued by mathematicians, and to employ equations in which the true longitude is the independent variable. Instead, however, of integrating the equations of motion by the method of indeterminate coefficients, as the author had proposed, M. Poisson recommends the adoption of the method of the variation of the elliptic constants. In the present paper, Mr. Lubbock states the reasons which have determined him not to employ the latter method, founded chiefly on the advantages of obtaining complete uniformity in the methods used in the theories